

Peer-review report of

Shackleford, C., Rabinowitz, M., & Richeson, J. A. (2026). Stigma-based solidarity betrayal: Implications for resistance to the rise of ethno-nationalism in the U.S. *advances.in/psychology*, 1, e349228. <https://doi.org/10.56296/aip00044>

Round 1

Editor Decision

Dear Authors,

Thank you for submitting your important work, “Stigma-based Solidarity Betrayal: Implications for Resistance to the Rise of Ethno-nationalism in the U.S.,” to our special issue on the Psychology of Pushback. We were fortunate to receive evaluations from two experts in the field, with core expertise closely related to the topic of the manuscript. We thank them for their detailed and constructive evaluation. We both read and evaluated the manuscript before consulting the evaluations.

Those of us who reviewed your work all see several clear strengths in your research: It breaks new ground through an original perspective, focuses on a critical context of democratic backsliding, presents two studies, is generally well written, and offers stimulating findings.

At the same time, the reviewers identify several issues that prevented them from supporting publication of the current version of the manuscript. These reviews are very constructive and describe changes that would make the manuscript stronger. Many of these can be addressed by some re-thinking and rewriting to bolster the conceptual case for the work and the interpretation and implications of the findings – including acknowledgement of limitations. Because the reviewers’ comments are detailed, we will not repeat each point here. Instead, we highlight what we see as the most important issues to address, and we ask you to consider each of the reviewer’s points in a revised version.

We agree with the reviewers that more detailed and explicit explanations of your logic in the derivation of the hypotheses is needed. While the Introduction does ground the current research in previous work on stigma-based solidarity, the connections need to be tighter and stronger. For example, work on the black-sheep effect is discussed, but there is not direct comparison between the intersectional in-group (White women) and out-group members in the study designs. While White men are a racial ingroup for participants (White women) they are described as “an advantaged societal group.”

Reviewer 1 finds the core concepts to be vague, and the U.S. context as being taken for granted. They further highlight that key design choices (e.g., which groups are compared) lack theoretical and contextual grounding, interpretations outrun the

mixed evidence, and basic details (i.e., construct definitions, preregistration, study dates, materials) are unclear. They, for example, observe: “There’s an implicit assumption that certain racial groups are expected to act in alignment, but there’s little justification for this expectation in the manuscript. I can understand this prediction, but the underlying logic is not explicitly unpacked.... The theoretical rationale for why White women (or Black women) would feel betrayed by a minority group’s voting behaviour needs much more depth.” Reviewer 1 further notes that “the measurement and operational definitions could be clearer. Several key constructs...are not introduced with sufficient clarity in the Introduction. It’s important to explain how these were operationalised and why they were chosen as key variables.” In addition, as Reviewer 1 explains, it is important to describe the U.S. context for the work more fully for readers unfamiliar with it, identifying the key social and political factors in that context.

Reviewer 2 further remarks that “in both studies, the policy support items lack theoretical grounding and were difficult to understand.” Reviewer 2 also believes – and we agree – that the logic behind the hypotheses and statistical analyses in Study 2 require better articulation. The Reviewer explains, “In Study 2, the comparison between White and Black women did not make sense considering the target groups: White men and Latinos. For White women, these groups provide the possibility to explore questions about relational betrayal vs. stigma-solidarity betrayal. But, for Black women both groups are outgroups, so the same type of comparison cannot be made. Thus, it’s unclear whether relational betrayal is unique to White women or would generalize to other groups.” Reviewer 2 thus states that Study 1’s vignettes confound target group and past voting patterns, Study 2’s target comparisons do not let Black women test “relational” versus “stigma” betrayal.

While the analyses are generally clearly presented and appropriate, like Reviewer 2, we believe that the rationale you present implies tests of mediation by betrayal rather than simply zero-order correlations. We ask that include tests of mediation in the revised version of your work (while acknowledging the limitations of inferences drawn from correlational mediation).

There are additional issues that also need to be addressed in the Discussion sections. One challenge of the current manuscript is that the results are “mixed” – the analyses do not yield unequivocal support for the predictions. All research has “loose ends,” but these can stimulate important future research in this area. The reviewers both indicate that this is a fertile line of work. We ask that you acknowledge the limitations of the findings and elaborate more concretely on the specific questions that you feel need to be pursued and how in future research to tie up these loose ends. In addition, in discussing limitations, please consider Reviewer 2’s point about the “confound between the conditions” and Reviewer 1’s concern that currently “there is limited reflection on constraints of generalisability. The U.S. context is unique in many ways, and that’s fine – but the implications of this uniqueness should be discussed.”

While the issues raised are challenging, we believe that many of these concerns can be addressed (or at least discussed) in a revision and, weighed against the novelty of the research, we invite a resubmission pending major revision. In a revision, please address each of the reviewers' specific points. In addition, please address the following:

- Please provide more information about the power analyses of each study and do so in the main text (they use different designs – thus the same power analysis cannot inform both studies).
- In Table 2, provide 95% CIs instead of SDs (this applies to means reported in the main text as well). Italicize “*N*” (make sure all Latin statistical letters are italicized in the manuscript). These comments also apply to Table 4.
- In Study 2, a figure displaying the means and 95% CIs for Roots of Betrayal would be beneficial.
- Always provide three digits for *p*-values, also if the last number is 0. Effects below .001 are correctly described as $p < .001$ as you currently do.
- Please avoid terms that could be understood as implying causality (e.g., “predicted”) when describing correlational relations.
- In Study 1, please also discuss the main effect of election outcome.
- In Study 2, it is not entirely clear how “Collective solidarity intentions” was measured. Was it measured twice or did the formulation differ between participant groups? If the latter is the case, why was it assessed as “women” and not “White women” for the latter group of participants?
- Study 2 included a second manipulation. This should be described already in the Introduction section, including the expected results and the results should be summarized briefly in the Results section.
- Prolific compensates in GBP. Are the USD descriptions converted?
- Stylistic issues
 - o Please capitalize headings in line with APA7. Some level formatting of headings also need revision to comply with APA7.
 - o In APA7, female is used as an adjective (e.g., female ... voters) and women as a noun.
 - o Please follow APA7 capitalization standards also for figure and table captions. Describe in figure notes what error bars represent. We assume 95% CIs.
 - o P. 18, it is not clear why [] are used. Statistics are better embedded in text with commas.
 - o Don't use “&” in text instead of “and.” We identified at least one such instance.
 - o Please move the text from all footnotes into the main text.

- o Unless referring to statistics, numbers under 12 should be spelled out.
- o Please make sure there is a space before and after a p -value. The same applies to η^2_p .
- o Generally, please proofread the manuscript for consistency in style and format (e.g., the use of subscript) and language.
- o There are several issues regarding citations and the reference list. Please make sure to provide DOIs as URLs for all references. The citation check highlights several additional issues (though you can ignore some false positives). Please thoroughly correct all. The report can be found here: <https://drive.google.com/file/d/1IUtZyZMn8IED3PulaAQ0OOjyyp-jkyjY/view?usp=sharing>

In conclusion, we appreciate your submitting this important work to our special issue. While we recognize the reservations of the reviewers, who are experts in this area and have provided thoughtful evaluations, we also share their view about the originality and promise of this line of research. Thus, we are offering you the opportunity to revise and resubmit the work for further consideration. While we cannot guarantee the outcome of that process, we would not offer this opportunity unless we believed that it is possible that, with appropriate revision, the manuscript could eventually be strengthened sufficiently to merit publication. That determination, of course, would depend on how well the revised manuscript addresses the concerns raised. We understand that these changes will require an extension of the word limit. Please feel free to use up to 3,000 extra words in your revision.

Given the nature of the revisions we are requesting, we ask that you submit a revised version within six weeks' time (i.e., by September 19). Please do not hesitate to reach out if you should require an extension of this deadline. Please outline in a revision letter how you addressed each point and provide excerpts that outline these changes.

Best,

Jonas R. Kunst & John F. Dovidio

Reviewer 1

Thank you for the opportunity to review: Stigma-based Solidarity Betrayal: Implications for Resistance to the Rise of Ethno-nationalism in the U.S. This work investigated what happens when expectations for solidarity are violated. Study 1 investigated the degree to which White women who supported Harris for president felt betrayed/surprised, trust, and solidarity toward the outgroup and the study design manipulated whether the outgroup was: White men vs. Arab men and manipulated whether the outcome was that: Trump vs. Harris won. All participants were told and the majority of the outgroup voted in favor of Trump. Study 2 investigated the degree to which White and Black women who supported Harris for president felt betrayed, surprised, reasons for feeling betrayed, and solidarity toward White men and Latinos.

The study manipulated whether participants were considering the dependent variables with respect to White men or Latinos in a within-subjects design. Across both studies, White women reported more betrayal toward White men relative to the comparison group (Arab or Latinos).

Overall, there is much to like about this work. It investigates a critically important topic. The introduction was well written and reviews a broad scope of the relevant work. I really liked the additional measure of reasons for feeling betrayed in Study 2. And the method/results were generally easy to follow in both studies.

Despite my enthusiasm, I have some concerns about the current manuscript.

In Study 1, there is a confound between the conditions. In particular, the information about Arab Americans indicated that the majority of them voted Democratic in 2020 whereas for White men it said that the majority voted Republican in 2020. This difference between the conditions leaves open the possibility of other justifications leading to the current findings.

In Study 2, the comparison between White and Black women did not make sense considering the target groups: White men and Latinos. For White women, these groups provide the possibility to explore questions about relational betrayal vs. stigma-solidarity betrayal. But, for Black women both groups are outgroups, so the same type of comparison cannot be made. Thus, it's unclear whether relational betrayal is unique to White women or would generalize to other groups.

Finally, in both studies, the policy support items lack theoretical grounding and were difficult to understand. Given the issues with the study design, the findings associated with these items were therefore difficult to interpret.

I strongly encourage the authors to continue to pursue this line of work. I hope these comments are useful in moving this work forward.

Smaller point – I did not understand the use of correlations between betrayal and future solidarity. Would it be better to test this question in the context of mediation models?

Reviewer 2

The paper presents a novel and creative set of studies investigating perceptions of political betrayal and stigma-based solidarity. The work is certainly timely and taps into important conversations around intergroup solidarity and coalition-building across marginalized identities. However, there are multiple significant theoretical, methodological, and interpretive issues that limit the contribution this manuscript can make. Below I summarise the key issues:

First, the theoretical clarity and framework seems to be lacking. The central concept of political solidarity or stigma-based solidarity and betrayal is not sufficiently developed. Terms like resistance are used but not adequately integrated in the theoretical framing. It's not clear what counts as a "coalition partner," nor what psychological mechanisms would underlie feelings of betrayal in the voting context explored here. A key question that kept coming up for me was: why should these participants feel betrayed?

There's an implicit assumption that certain racial groups are expected to act in alignment, but there's little justification for this expectation in the manuscript. I can understand this prediction, but the underlying logic is not explicitly unpacked. Importantly, racial groups are not monoliths, and they encompass a wide range of political beliefs. The theoretical rationale for why White women (or Black women) would feel betrayed by a minority group's voting behaviour needs more depth. Unfortunately, at present, the reasoning feels thin, and the core concepts remain quite vague.

I have always liked the stigma-based solidarity research and found it compelling and promising. That said, more work is needed to connect the dots between this framework and the current research questions, especially in the Introduction and General Discussion. There is a need to dig deeper into what solidarity means here, how it's expected or perceived, and when and why its perceived absence might result in betrayal.

Second, there are oversights in the contextualisation of the work. The paper mostly assumes a reader familiar with the nuances of the U.S. political landscape. As our field becomes increasingly international, it's crucial to provide clearer context for non-U.S. readers. Why was the U.S. chosen for this work? What is it about the Harris and Trump voter groups that makes them meaningful in relation to the psychological processes being studied? How does it provide a suitable context for theoretical development?

Even more importantly, there is limited reflection on constraints of generalisability. The U.S. context is unique in many ways, and that's fine – but the implications of this uniqueness should be discussed. The authors do mention wanting to inspire research "in other socio-political and cultural contexts," but this feels too vague. Without a more explicit discussion of what features of the U.S. context matter (and why), it's hard to see how this work can contribute to broader theory.

Third, there are issues with the methodological decisions and conceptual choices made in both studies. A number of choices in the study design raise important questions that remain unanswered in the manuscript. Why were White women who voted for Harris chosen as the focal group? This is a key design decision that requires clear justification. Why were White men and Arab Americans selected as the comparison targets? The choice to specify gender in one group but not the other is puzzling and not theoretically grounded. These categories are central to the study but not sufficiently unpacked conceptually.

If the goal is to understand intergroup betrayal, why are interpersonal constructs (such as interpersonal ties or relationship science) being used over frameworks like social identity theory and the literature on intergroup processes? They seem much more directly relevant for understanding the group-based dynamics this paper is focused on. For example, White men are racial ingroup members for White women, so the finding that they were perceived as more betraying could potentially be explained through this lens, but this angle isn't explored. A deeper engagement with social identity dynamics would strengthen the analytical lens.

In short, I worry that the theoretical choices do not appear to align well with the methodological decisions. This mismatch limits the interpretability of the findings.

Fourth, the interpretation of results and general discussion has some limitations. While the studies yielded some surprising and interesting findings, the interpretation of those findings felt overstated at times. For example, the claim that “the present findings underscore the fragility of coalitions based on shared marginalization” seems much stronger than what the data can support. What exactly is fragile, and in what way?

The discussion would benefit from a more grounded interpretation of the mixed results. Abstract statements like “broadening the psychology of betrayal to include intriguing dynamics” are difficult to evaluate because the “dynamics” referred to are not clearly defined.

What's currently missing is a clearer articulation of what we can, and cannot, conclude based on these studies, and what the theoretical contribution is.

Fifth, the measurement and operational definitions could be clearer. Several key constructs, such as “surprise,” “collective vs. individual betrayal,” and others, are not introduced with sufficient clarity in the Introduction. It's important to explain how these were operationalised and why they were chosen as key variables.

Finally, I have a few minor points and clarifications:

It was not clear whether the studies were pre-registered. Could the authors clarify?

In Study 2, it would be helpful to explicitly state whether the exit polling figure used was based on real data or was fabricated for experimental purposes. It seems to be based on real data, which is interesting, but a short explanation of how it was developed and presented would be valuable.

The OSF materials are appreciated, but I was unable to find the polling figure mentioned in the manuscript within the Qualtrics document. It would be important to include this.

The manuscript states at different points that Study 1 was conducted “just prior to the election” and Study 2 “just after the election.” It would be useful to include the exact dates or timelines for both studies.

Overall, a combination of issues (underdeveloped theory, limited contextualisation, ambiguous design decisions, overextended interpretations) make it difficult to evaluate the true contribution of this work at this stage.

Thanks for the opportunity to review this interesting work. I hope that the authors find these comments useful as they develop this work further!

Round 2

Editor Decision

Dear Authors,

Thank you for submitting your revised article, “Stigma-based Solidarity Betrayal: Implications for Resistance to the Rise of Ethno-nationalism in the U.S.,” to our special issue on the Psychology of Pushback. We received evaluations from the two original experts, and we also evaluated the new version independently.

We all find the paper greatly improved and thank you for your diligent work. The idea remains original and important, and the context and presentation of the results are now much clearer. Reviewer 2 recommends acceptance, whereas Reviewer 1 has remaining concerns. While Reviewer 1 may not be fully convinced (which is a natural part of scholarly discourse) and raises specific concerns, we believe that the manuscript has a good likelihood for acceptance with some additional, relatively minor revisions. Of course, the final decision will be determined by how effectively you address the remaining issues. Unless something unexpected arises, we do not anticipate seeking any additional outside review.

In revising the manuscript, we suggest you focus on the following regarding Reviewer 1’s comments:

- Please present the hypotheses tested more explicitly, preferably in a numbered format, along with their rationale in the introduction of each study.
- Report the results for the threat vs. promise manipulation in full in the SOM and provide *p*-value ranges in the main manuscript.
- Despite attempts to soften the claims, the discussion still appears to overstate generalizability and cross-cultural applicability, particularly given the mixed and sometimes non-supportive findings. As you did not replicate results across cultures, this claim is quite bold: “we expect the betrayal of stigma-based solidarity to be broadly applicable to dynamics among different groups

that face societal marginalization, irrespective of the specific national or cultural context.” Please reformulate this more carefully and modestly.

- In the limitations section, please revisit the potential design confounds (gender, race) in more detail. Propose specific future research designs that could more effectively disentangle these factors.
- Also in the limitations section, please briefly note the high exclusion rate that Reviewer 1 refers to, offer an explanation for it, and alert readers to potential implications. We understand that this may have been due to predefined inclusion criteria, but it would be good to outline the rationale more clearly.

In addition to these main points, please address the following minor issues:

- You write: “We also assessed participants’ support for one protective policy (anti-discrimination laws) and one punitive policy (domestic surveillance) targeting the focal marginalized outgroup (Arab Americans) as another index of (stigma-based) solidarity intentions.” It is somewhat unclear how this operationalization captures “solidarity” and “intentions.” Please consider using different terminology here.
- “1000 White participants who identified...” – Per APA 7 style, numbers that begin a sentence must be written out. This occurs in at least two places.
- Study 1: Please visualize the significant three-way interaction for Collective solidarity intentions.
- In the introduction to Study 2, please explain in more detail the rationale for focusing on both Black and White women. This is briefly mentioned at the end of the study's introduction, but as a major design change, it would benefit from more thorough elaboration.
- When reporting simple contrasts following ANOVAs, please include Cohen’s *d*.
- Please do not italicize Greek letters (some eta symbols are still presented in italics).
- Please report the statistics for all tested non-significant interactions.
- Please provide figures in SVG format to ensure high resolution during the production process. These can be exported directly from R and then inserted into the word document.
- We noticed a few typos (e.g., “...negatively affecting targeting...” and “A number of compelling implications that stem from this research.” which is missing a verb). These are not numerous and will likely be caught in a final proofread.
- Unfortunately, significant issues remain with your citations and reference list (e.g., missing references, incorrect years). To avoid further delays in the review process, please **thoroughly** address every issue detailed in this

report: https://drive.google.com/file/d/1n9x1PLOV-j_kkZKrl9HbA2yrX1EfXhr-/view?usp=sharing

Given that we are requesting minor revisions, we ask that you submit your revised version within three weeks (i.e., by December 26; please contact us if you need additional time given the upcoming holiday season). In your revision letter, please outline how you addressed each point. Importantly, please also provide excerpts that show these changes (this was missing from your previous letter). That you again for submitting this innovative work to our special issue.

Best,

Jonas R. Kunst & John F. Dovidio

Reviewer 1

Thank you for addressing the comments. I believe the authors address my concerns in the current version. I am happy for this work to go forward.

Reviewer 2

I appreciate the authors' efforts in revising the manuscript! The revised version is considerably stronger, particularly in its expanded theoretical framing and clearer articulation of some constructs. There is much to like in the research, given its focus on naturalistic contexts and groups and the novelty of the focus on betrayal in the stigma-based solidarity literature. However, several of my initial concerns remain unaddressed, and the revision has also raised new uncertainties regarding the data, methods, and interpretations. I outline these below and hope that they are helpful for this line of work.

The relational ties argument remain unconvincing. While the revised introduction includes discussion on normative expectations around loyalty, it is unclear how betrayal from a relational obligations perspective explains the findings from Study 1. The manuscript mentions that "Most White women have close ties to White men— as partners, fathers, brothers— which may generate expectations of care that extend to political support". This section was not substantively revised. This argument still lacks theoretical depth. The sole reference is to Burgoon (2015) and the underlying logic could be unpacked.

There is also a conceptual inconsistency. The relational ties perspective is discussed under stigma-based solidarity in the Introduction but treated as a distinct phenomenon in the discussion sections. The item used to measure "expectations for relational care" ("White men should know that a second Trump administration will be

disastrous for women”) does not clearly tap into the interpersonal ties argument provided. It is therefore unclear whether the data provides support for this argument.

Further, while the authors have chosen Arab Americans and White men as the focal outgroup due to the voting blocks that were visible – and I appreciate the authors explanations of this in the response letter – the gender confound makes the results difficult to interpret. The conditions do not seem comparable when gender is salient in one group but not the other. This is especially a concern when the study focused on White women’s marginalisation based on gender.

Although the authors have added explanations for the variables used, the justifications remain unclear in both Study 1 and 2. For the most part, it is mentioned that these constructs were measured, however it is not clear how they relate to the aims of the research. For example, it is mentioned that betrayal is examined at the personal and collective levels of self-construals, but it is not explained why this distinction is theoretically important and what the expected results would be. More generally, hypotheses related to the various dependent measures in both studies are not described explicitly (e.g., policy opposition, solidarity intentions). Relatedly, while the revised manuscript makes the bivariate correlational results clearer, it raises the question of what pattern of findings would support stigma-based solidarity betrayal. The analyses are framed as hypothesis-testing but appear exploratory. As a result, it is difficult to know what we can take away from the findings. Together with the mixed results across the studies, the results feel too preliminary to support the conclusions provided. This remains my main concern with the manuscript.

There were some methodological and design issues that the revision brought to line. I was surprised to see that there was another manipulated factor in study 2 that I don’t believe was mentioned in the main text in the original manuscript (perhaps only as a footnote). It is now mentioned that the manipulation of threat vs promise in study 2 did not show significant effects and was dropped from analyses. For transparency, I think the statistics should be reported. The design of the study should also be made clearer in the method section and the null effects should be discussed.

I also found it surprising that over 600 participants were excluded from Study 2 for not meeting selection criteria. This raises several questions, including the inclusion and exclusion criteria used, how participant recruitment occurred (were Prolific pre-selected categories/filters used? What measures were examined to determine exclusions?) and the full demographics of the sample initially collected.

In the general discussion, there is an effort to tone down the conclusions though some take-away points were still too strongly worded. For example, it was noted that it is expected that betrayal of stigma-based solidarity would be broadly applicable,

irrespective of national or cultural context. However, in the studies, many findings were not consistent with stigma-based solidarity betrayal – making it unclear whether such a claim is supported by the data. Similarly, the response letter notes that the revised manuscript discusses how the dynamics found “could manifest differently in other political systems of cultures” – but the discussion section does not seem to meaningfully address this, and my initial concerns remain. The response letter also notes that the authors do not “make any claims about the generalizability of our findings to other groups” – however the general discussion section does make such claims. Minor point: the manuscript title includes “implications for resistance to the rise of ethno-nationalism,” but this does not reflect the core take-away from the studies.

Thanks for the opportunity to engage with this interesting research.